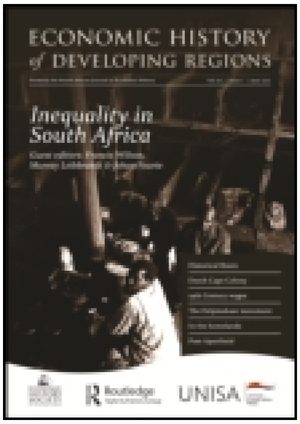

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A Clash of Disciplines? Economists and Historians Approaching the African Past

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DEBATE

A CLASH OF DISCIPLINES? ECONOMISTS AND HISTORIANS APPROACHING THE AFRICAN PAST

Morten Jerven*

ABSTRACT

This review article examines the differences in the approaches taken by economists and historians when interpreting social and economic change in the African past. It is argued that it is a mistake to assume that one discipline has supremacy over the other, let alone monopoly, when it comes to evaluating historical causes of African poverty. One of the shortcomings of the 'New African Economic History' is that it has largely sidestepped the issue of data quality. In cross-disciplinary work it is generally advised that data points and observations should roughly cohere with the state of knowledge in the other disciplines. Economists do themselves a disservice if the only criteria they consider for 'robustness' of historical arguments are those pertaining to econometric methods.

JEL code: N01
Keywords: Africa, causal history

1 INTRODUCTION

A 'New African Economic History' has been welcomed by Hopkins, the leading exponent of the 'Old'. The new literature has come from economists, 'unbeknown' to historians and without the established economic historians being in the driver's seat or providing the new arguments (Hopkins 2009, p. 155). Over three decades ago, in 1987, Manning called out: 'Avanti! Economic Historians!', only for interest in African economic history to diminish. Development economists have recently declared that 'History Matters' (for two different views on how history matters see Nunn 2009 and

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Woolcock Szreter and Rao. 2009),[2] and economic history stands to be invigorated by this new trend in development economics. It was previously lamented that economists took their starting point as 1960, as if events preceding this point in time had no effect on current development (Hopkins 1986; Manning 1987, p. 50; Austin 2007, p. 12). There was an informal division of labour between the disciplines: economists studied the post-1960 period, while historians were mainly occupied with the pre-1960 period.[3] Eight years ago, Ellis issued a call for historians to write 'the history of contemporary Africa', which partly reflects that 1960 is becoming a more distant part of the past (2002).[4] The disciplines are thus finally intersecting, if only in terms of the time period studied.

There may, therefore, be good reasons to be excited about the fruits of cross-disciplinary work. But there is also good cause to be aware of the potential for conflict and misunderstandings, as a recent contribution by Dr James Fenske (2010) in this journal has displayed. 'Historians' and 'economists' differ in the types of questions they are interested in, how evidence is dealt with, the role of theory and models – or to put it simply: there are important methodological differences between economists and historians.[5] This review article seeks to examine some of the main differences in approach between economists and historians as they interpret social and economic change in the African past. It first attempts to uncover points of divergence, and then ultimately the paper searches for common ground. Parts of this article can be read as a direct response to Fenske, who while providing a very thorough and useful review of the 'New African Economic History', makes some controversial claims. Particularly, the article responds to Fenske's arguments regarding 'causation' and 'data quality'. In turn, the responses to these arguments raise others, such as problems with the use of 'instrumental variables' and 'path dependency' arguments, but it should be stressed that these are part of a wider discussion.

Fenske claimed that, 'practitioners of causal history focus on identifying causal historical relationships, which sets them apart from the qualitative 'old' economic

2 Since the seminal contribution by North (1990) it was held that 'Institutions matter' for development. The 'Institutions matter' mantra could be read in two different contexts. First, as a reaction to the old mantra of structural adjustment 'getting the prices right' and thus related to the appearance of 'Governance' as a catchphrase. Second, it can also be juxtaposed to the competing claim that it is geography that matters as an explanatory variable in long-term development patterns, as for instance in Rodrik, Subramanian and Trebbi (2002) 'Institutions rule' versus Sachs (2003) 'Institutions don't rule'. In the latter interpretation, 'History matters', can of course be read as a synthesis: both institutions and geography matters. For economic historians this is indeed old news, and Findlay and O'Rourke (2007, p. 470) call it a cliché.

3 This is a very crude generalisation, and the list of exceptions is very long.

4 For a recent survey see Cooper (2008). By now major post-1940 and post-1960 histories has been written by historians (like Cooper 2002 and Nugent 2004), whereas earlier post-colonial histories tended to be written by journalists (like Meredith 2006).

5 For a classic statement of the debate see Fogel and Elton (1983).For a very recent contribution see, Morck and Yeung (2011).

historians and from much of the 'new' economic history' (2010, p. 180). This is a misstatement of the issue. Iliffe's *The African poor: a history* (1987) is an instructive example. It is a work that is fundamentally interested in unearthing historical causes of poverty. The crucial difference is that in order to address that question, Iliffe proceeds to trace and define the different forms that poverty has taken through time and space in Africa. In this interpretation, poverty is multidimensional,[6] and therefore it defies simple quantification and advanced econometric testing. The multidimensional nature of poverty is particularly true for a continent-wide historical examination. First of all, the appropriate data is not available, and even if they were, they would potentially capture both what Iliffe would call structural poverty and conjunctural poverty, which crucially is argued to have different causes. Iliffe, by examining the history of poverty, identified plausible patterns of causality, without using historical observations on poverty to determine causality statistically. There is a difference between historical significance and statistical significance. It is suggested here that 'New African Economic History' is better served by paying attention to both, rather than arguing for the supremacy of the latter.

In a critical review of the 'reversal of fortune thesis' (Acemoglu, Johnson and Robinson 2002),[7] Austin (2008) suggested that it may already be the most discussed contribution to the study of the economics of growth and development since Lewis's model of development, 'with unlimited supplies of labour'. The comparison is instructive. In a dual sector model Lewis postulated that there was an untapped labour supply in the rural agricultural sector, and derived from this a possible model for future economic development. This model has been applied empirically to explain particular African historical economic developments, such as in Barber's study of Rhodesia (1961). That specific application of the model was subject to a sharp and still relevant critique by Arrighi (1970, p. 227):

> Barber exemplifies the ideological bent of the anti-historical approach which is the essence of modern economics. For in economics assumptions need not be historically relevant. In fact, they are often plainly untrue and recognized as such. Historical processes fall into the background and are summarized by statistical series of ex-post data, the 'stylized facts' as they are sometimes called, which by themselves reveal nothing about causation. Thus, all that Barber takes from the complex historical process which we have been analyzing are a series of real wages and a series of rates of African participation in the labour market. Causal relations, on the other hand, are not derived from historical analysis, but are imposed from without, that is, through *a priori* analysis: and a set of assumptions which yields the stylized facts is held to have explanatory value, irrespective of its historical relevance. But since there will be normally many such sets, this methodology leaves room for considerable arbitrariness of choice and therefore mystifications of all kinds. In view of this, the low scientific standards attained by modern 'development economics' should surprise nobody.

6 It is fair to say this is the mainstream scholarly interpretation of poverty, outside economics. This interpretation does of course owe a great deal to the cross-disciplinary work of Amartya Sen (economics and philosophy).

7 Henceforth AJR.

How do the recent contributions from development economics compare to classic models such as the model of ‘unlimited supplies of labour’? The Lewis model used a few simple stylised facts to gain insights and derive policy implications regarding conditions for future economic development. The model was logically deduced from assumptions. These assumptions could be argued to hold or not, and the model could be deemed applicable or inapplicable to specific empirical studies. It was not offered as a grand explanation of historical development patterns, induced from empirical ‘evidence’, or presented as the outcome of a ‘natural experiment’, unlike many of the recent contributions from ‘New African Economic History’. It also had some direct policy implications, such as those regarding barriers to mobility of labour. Recent contributions are famously weaker in that department: Crafts (2004, p. 53) recounts ‘North’s jocular advice that Russia’s best chance of successful economic development was ‘to get a new history’’.

There are many issues in ‘development economics’ and ‘African history’ to which this article will not be able to do justice.[8] The present question is whether economists treat history in a manner which is compatible with the approach of historians. The article will first offer a history of how ‘history’ entered the equations of economists, before some fundamental questions about evidence, theory and cross-disciplinary method are considered. The article is searching for common ground, but this can only be achieved after firmly establishing the principles upon which disciplines ‘agree to disagree’.

2 THE PRESENCE OF THE PAST IN DEVELOPMENT ECONOMICS

Why does a discipline that is occupied with current developments concern itself with history? Jenkins (2006) suggests three reasons. First, the study of economic development is coming of age, and is having its own intellectual and practical history of failures and successes. Second, there is a long standing use of historical parallels or ‘stories’ in order to validate development theory. Third, economists associate history with the importance given to ‘path dependence’ in explaining economic outcomes. On the latter, Jenkins (2006, p. 7) comments ‘when faced with studies by economists who use history mainly as a source of data with which to advance unsubtle hypotheses concerning the causes of developmental outcomes, those who had earlier called for scholars to pay more attention to history may regret ever having voiced such a plea, and find themselves revisiting the proverb about being careful what one wishes for’.

8 This article complements some reviews by economic historians Manning (1987), Hopkins (1986, 2009) and Austin (2008) and by the historian Cooper (1993) of different contributions by development economists using the African past to understand the present. Recent reviews of the cliometric contributions by economists such as Bhattacharyya (2009), Nunn (2009), Ray (2010) and Fenske (2010) and their valuable contributions in summarising and organising that literature do not need to be repeated here.

Interest in the history of economic growth is not new. The more pressing question is why the sudden interest by mainstream growth economists in the (very) long term history of poor countries? To understand the particular function 'history' takes in the 'New African Economic History' it is necessary to review the path of growth accounting, from the early 1990s to the recent arrival of very long term investigations of economic development.

Arguably, a seminal moment in development economics was the availability of the Penn World Tables following the work of Robert Summers and Alan Heston (Stern 1989, Deaton and Heston 2010). The boundaries of the historical investigation for development economists were effectively set by these datasets. Paul Collier wrote in 1993, 'Africa offers the imminent prospect to applied macroeconomics of perhaps thirty national time series data sets' (p. 60). Barro provided the seminal article and method of investigation (1991). In short, that article presented a cross country growth regression model, making use of the availability of global data on growth in GDP per capita, from 1960 onwards. One of the central findings was a large and significant negative African dummy variable. Barro's interpretation of the dummy variable was that the analysis had not yet fully captured the characteristics of a 'typical country' on the African continent (Barro 1991, p. 437), then followed what a critic called the 'Quest for the African Dummy' (Jerven 2011c).

Over the following decade, a range of contributions attempted to 'solve' the global economic growth equation by finding quantifiable variables that could explain the apparently anomalously slow growth of Africa. This voluminous literature has been called the 'growth regression industry' in which 145 explanatory variables were found statistically significant (Durlauf *et al*. 2005).[9] The immediate focus was on factors of growth, such as availability of capital and the quality of labour. The growth models were later expanded to include measures capturing policy such as inflation rates and black market premiums and institutional quality proxy variables (Collier and Gunning 1999a, 1999b). Increasingly, it was perceived that these particular policy choices and matters of institutional quality needed explaining in themselves (Temple 1998). A chronic failure of growth in post-colonial Africa became an accepted stylised fact (Collier and Gunning 1999a), and it was not the growth rates themselves that needed explaining, but rather the parallel outcome of a permanent growth shortfall and failed states/policies. According to this assumption, a lack of growth in the past is manifested in a low income today. Thus, the growth literature moved to focus on explaining cross-country inequality, as measured by *per capita income* today, rather than observed patterns of past *per capita growth*. That a current income distribution can be roughly equated to

9 Kenny and Williams argued that despite the scholarly effort 'current state of the understanding about causes of economic growth is fairly poor' and that 'we are in a weak position to explain why some countries have experienced economic growth and others not' (2001, p. 15)The argument for pleading ignorance was recently summarised by one of the central participants in the growth regression industry, William Easterly, in a lecture held at London School of Economics, 19 May 2010: 'We don't know how to solve global poverty, and that's a good thing'.

a historical chronic failure of growth in African countries is a large leap of faith, upon which this long term growth literature is built.[10] Furthermore, using regression models has meant that little notice has been given to the exceptions that quite possibly break the rule. Rather, the econometric growth literature takes comfort in that the R^2 does indicate that the causal relation holds on average (Austin 2009b). Specifically, the caveat of relying on the stylised facts of the African growth failure has not been subject to real empirical justification. Does a global sample of average economic growth between 1965 and 1995 appropriately capture the African post-colonial development experience? This is often an unstated caveat in the growth literature.[11] An alternative interpretation of the post-colonial growth record would be that growth was widespread and rapid from the 1950s into the 1970s, followed by a period of stagnation until the 1990s, after which growth recovered.[12] Such an alternative stylised fact would call for different explanations.

There is a similar increase in the literature aiming to identify the 'root of African underdevelopment' today, as there was in the literature hoping to eliminate the 'African Dummy Variable' two decades ago. Then, a large set of equally plausible, but often mutually exclusive factors were found. Responding to the inability to reach consensus, the empirical growth literature went on to try to account for the current national income level distribution. This shift raises the question of whether or not we can take it as given that the income per capita today captures a century or five of economic development.[13] These are large leaps of faith, and arguably 'compression of history' is a better label than 'causal history' for this new trend in the econometric literature.[14]

10 There are some very good reasons why we should use doubt rather than faith in social science. One is methodological. It is a noted problem that mainstream economics seldom adheres to the principles of Popperian falsification (Stein 2008, p. 68), and that the main preoccupation is to establish plausible relationships rather than discarding them.

11 Though Collier and Gunning (1999a, p. 79) pointed out that 'one limitation of the growth regression literature is that to date it has focussed upon explaining long-term average African slow growth', it did not affect the parameters for empirical analysis.

12 As argued in Jerven 2011a and 2010c.

13 Jerven (2010a) establishes that the income per capita data varies hugely between the different datasets and, on that basis, concludes that it is futile to use GDP estimates to prove a link between income today and existence of pro-growth institutions in the past. It is further argued that no one dataset is decisively superior to the others, thus it is not only a matter of deciding which one to use, but rather that a claim of robustness needs be established with regards to all the competing datasets.

14 It was Austin (2009) not Hopkins who used the term 'compression of history'. Curiously, Fenske (2010) did not reference Austin, when suggesting the term 'causal history' in response.

3 'CAUSAL HISTORY' OR 'COMPRESSION OF HISTORY'?

One important motive for the use of historical variables is technical: to facilitate the use of instrumental variables to take care of the 'endogeneity problem'. The latter arises when factors that are supposed to affect a particular outcome, depend themselves on that outcome – which was exactly the difficulty encountered by the scholars attempting to quantify the effect of aid, infrastructure and corruption on development. AJR's seminal contribution was to use European settler mortality rates as an instrument for risk of capital expropriation. Development 'today' obviously does not have a causal effect on settler mortality a long time ago. It is this innovative use of history, to avoid reverse causality, that has given the paper such a high standing in economics departments. Yet, the method has its critics. Deaton (2010) warns that the users of instrumental variables often imply that the variable is 'external' to the question at hand, when in fact it is simply 'exogenous' – that is external to the model, yet not truly external to the question the model grapples with. This makes intuitive sense in the example of settler mortality as it was probably related to climate and diseases, which may also affect development today.[15]

Hopkins (2009) and Austin (2008) caution that the data used in the regressions, specifically the historical population estimates in AJR, are weak. [16] Fenske, counters: 'These critiques are misplaced. First, the reliability of the McEvedy and Jones' (1978) population estimates is an unimportant distraction. If these are measured with unsystematic error, they would bias the results in AJR (2002) towards zero; using classical measurement error, their results understate their own case.' (Fenske 2010: 190). Hopkins notes that recent work has revised population data upwards. But Fenske counters that the problem could be solved by undertaking 'an additional robustness check that might take an afternoon including data entry'.

This misses the point. First, the population data is not randomly distributed; it is argued that there is a systemic downward bias in the estimates.[17] The biggest problem, however, is that this type of defence is by extension arguing that the robustness of a historical argument is subject only to econometric criteria. Certainly the robustness of the model and its internal validity can be tested, using different control variables and running the model using alternative datasets. However, establishing the validity of these population data is a matter of historical evidence, and African historical demography is worth more than an afternoon's attention.

15 Among many, Bloom and Sachs (1998)have argued has direct causal effects on income today.

16 The data on settler mortality has also been subject to criticism and shown not to be robust to other justifiable data points (Albouy 2008), note that AJR (2005) has replied.

17 Manning (in Ittmann, Cordell and Maddox 2010) has recently argued that the pre-colonial populations may have been twice as large as indicated in most datasets. Note that the main issue here is not direction of bias, but rather lack of rigour when discussing the evidence.

The point of contention here is whether one should consider 'New African Economic History' as cross-disciplinary work. Specifically, should the 'reversal of fortune' thesis be read as an economic model or as a historical argument? Is it saying, 'Let us assume that population was so and so, and it would then follow . . .' or 'it has been established that population was 'X' and therefore 'Y' follows'? The term 'causal history' implies the latter. A cardinal principle of cross-disciplinary work is that the scholar who is doing the work makes sure that an argument or empirical observation should be coherent with the state of knowledge in that discipline.

This argument extends to other evidence besides population data. It has already been noted that the use of the income and growth data relies on probability, stylised facts and faith, rather than empirical justification. Of particular curiosity is the use of data from the 'Human Relations Area Files' or the *Ethnographic Atlas*, both created by George P. Murdock, in regressions in lieu of evidence on institutions,[18] without comment on its historical validity, or its current standing as evidence in the discipline it originated: anthropology.[19] As Austin (2009) notes, its use in historical arguments is doubly ahistorical. The observations on institutions in the dataset need to be historicised, not only by dating when the observation was made, but also with regards to the discipline itself, in terms of what is considered acceptable evidence.[20]

A central premise in African history is that 'the visions of Africa often derive from Europe and come still predominantly from the Western World. Our perception of the African past has always been a European perception' (Vansina 1986, p. 40). Therefore, when interpreting social and economic change of African societies, the criticism of sources is particularly important. Administrators and explorers preceded scholars in making observations; these observations were made using pre 1900 categories. The early scholarly observations are also dated. This placed constraints on what type of information was gathered and how it was categorised. The basic question is whether the knowledge gained through these sources is at all useful. These are recognised problems in the African history discipline, but they are in dire need of re-stressing if economists are seeking to contribute to the interpretation of the African past.

Without doubt the biggest challenge for African history writing has been a dearth of reliable evidence. In turn, the ingenious solutions to this challenge have grown to be

18 Either the effect of institutions 'X' (as in Bolt, et al. 2009) or as an outcome 'Y' (as in Fenske 2009), using data from *Ethnographic Atlas*.

19 In email correspondence regarding the 'Human Relations Area Files' the LSE anthropology professor Chris Fuller described them as 'a blast from the past' and noted that apart from Jack Goody the files were not highly rated among anthropologists in the UK . Tobin summarises the critiques of the database made in the 1960s and 1970s and notes that 'critiques have grown rarer recently, not, I think, because of the heirs to Boas, Benedict, Leach, and Geertz have grown less antagonistic to quantification and comparison, but because, if they think of HRAF at all, they tend to think of it as moribund' (1990, p. 478).

20 Similar concerns apply to the use of the dated and crude proxies for 'social capital' (as in the Adelman and Morris dataset in Temple and Johnson (1999)) and ethnic fractionalisation' (as with the index computed from Atlas Naradov Mira in Easterly and Levine (1997)). Fenske (2010, p. 183)) notes that the latter dataset has been coded by Posner (2004) to capture 'politically relevant groups'. That may raise more problems than it solves when considering it as historical evidence.

one of its strengths. When a longer time perspective is adapted to the econometric study of African economies, the lack of quantitative evidence limits the types of questions that can be asked and answered and we may end up with 'compression of history'. African economic data are limited both in availability and quality (Jerven 2010b, 2010c).[21] Furthermore, the datasets are biased in two respects: we know less about economic change before than after 1960 and we know much more about exports than production (Jerven 2010a, 2010b). Thus, exclusive use of quantitative approaches may mean that we understate the importance of economic change in the early period, while ignoring internal economic dynamics in the whole period. [22]

Any African economic history investigation must both begin and end with a critical analysis of the quantitative data. It must further be supported by careful qualitative evaluation. The quest for quantitative resolution must be enriched with qualitative rigour.[23] Despite the repeated declaration of the mantra that 'history matters', there persists, if not an anti-historical bias, then a serious neglect by development economists, of the qualitative historical literature and the historiographical lessons drawn during the existence of African history as a discipline.

4 CONCLUSIONS: MISUNDERSTANDINGS, COMMON GROUND AND AN AGENDA FOR MIXED METHODS

What could perhaps best be described as an encouraging but qualified welcome by Hopkins (2010) to the field of African Economic History has seemingly been brushed away by Fenske (2010), who argues that econometric techniques have supremacy in investigating historical causal relationships. It is a misunderstanding that historians are not interested in causality and the clearest statement of that misunderstanding is provided by Fenske as 'If X causes Y, this is no less the case if X and Y are centuries apart' (2010, p. 190). Cooper suggests a typology of doing history ahistorically: 'story plucking, leapfrogging legacies, doing history backward, and the epochal fallacy' (2005, p. 17). When 'X' and 'Y' are centuries apart it radically increases the probability of 'leapfrogging legacies'.[24]

21 For further case studies of the problems arising from data quality problems in economic growth analysis see Jerven 2010d a (on Botswana), Jerven (2011b, on Kenya) and Jerven (2011c, on Tanzania). For an attempt to historicise the African national income data see Jerven (2011d).

22 A striking example of the power of datasets and the importance of vantage point comes from the debate on global warming. Maslin (2004, p. 27) argues that it took so long to notice global warming because of weaknesses in the global mean temperature dataset. From the viewpoint of the 1970s it looked like the temperatures had been falling since 1940s, but the current dataset, covering 1860–2010 shows that the temperatures are indeed on an increasing trend.

23 As argued by Harriss (2002).

24 Taking 'Y' – the income level today – as the starting point is in essence 'doing history backwards', while when picking the legacy 'X' the 'epochal fallacy' is near. Acemoglu and Robinson (2010) arguably provided a perfect example of 'story plucking' when they addressed 'Why Africa is poor?' in the previous issue of this journal, that is: picking historical examples from different times and places that supports the general claim without engaging fully with the historical context which these examples are taken from.

The basic intuition of the econometric models that propose ‘history matters’ is that historical events have lasting impact, as indicated in the term ‘path dependence’. A social or political settlement remains unchanged, or even stiffens over time, as indicated in the term ‘institutional sclerosis’. Because the essence of history is the study of the processes of change, the starting points of most historical studies can appear to be the very antithesis of economists’ long term explanations. This is not because historians are uninterested in causality; it is because of the concern that ahistorical approaches such as ‘leapfrogging legacies’ may fail to correctly establish causality.

Summarising the new long-term growth literature, Ray (2010, p. 50) suggests that what he calls ‘national infrastructure’ can explain a permanent state of global inequality. It has been argued that early institutional configurations set the patterns for long term growth through ‘path dependence’, so that an historical event ‘X’ caused outcome ‘Y’. The list of suggested historical events, or aspects of these, that had the decisive causal impacts is already long and is still growing. Nunn concludes that ‘the literature has made considerable progress in showing that history matters, what remains less well understood are the exact channels of causality through which history matters’ (2009, p. 31).

When asked to explain what path dependency really is, economists and business scholars reach for the famous example of ‘QWERTY’ (as in Reder 2003). That economic and institutional pressure has succeeded in keeping this particular keyboard version, used to type this essay, makes intuitive sense.[25] But it is a rule which has many important exceptions. Ray, pointing to the work of AJR (2002) and Engerman and Sokoloff (2002), argues that the explanation for the divergence in income levels today is ‘situations of stagnation in which the losers (or potential losers) control institutions. Losers defend an old system – likely one born under a colonial umbrella – and so impede progress’ (2010, pp. 56–57). Ray goes on to argue that even when winners are granted control, ‘they may block all redistributions that spread the growth process to other sectors’. In order to analyse causes of divergence ‘it will be of great importance to build a useful taxonomy of institutional performance (and reactions to such a performance) depending on who has control’ (Ibid).

This is an admirable ambition and seems to be one where some common ground potentially can be reached. Building a useful taxonomy would certainly require mixed methods, drawing on the expertise of economists, historians, anthropologists and others. The importance of institutions in explaining policy choice in post-colonial African economies was perhaps best illustrated by Bates (1981). His influential political economy model attempted to explain why some African countries tended to have a policy regime that favoured agricultural exports, while other discriminated against

25 One can apply of course also apply this argument to academic disciplines. Although it is not always the best practice, individual scholars tend to conform to common practice. Herein lies the promise of cross-disciplinary work.

them. The institution in question here were the marketing boards, a colonial innovation, but how the marketing boards were used depended on post-colonial political economy. Thus, path dependency from a colonial legacy did not uniformly apply, nor did it last permanently, as structural adjustment led to institutional reform. The irony of path dependent arguments is that they invariably turn out to have limited temporal validity.[26] The lesson is that the classifications of institutions or other economic evidence need to be fully historicised.

The concern about the 'compression of history' is not only a theoretical disciplinary debate on what constitutes a correct approach to history, it also has large bearings on the resulting policy implications. History certainly matters, but it is perhaps the cases where difficult historical legacies were overcome, even temporarily, from which useful policy advice can be drawn.

Fenske shows that the 'New African Economic History' is rich and diverse in empirical studies. This is indeed good and interesting news. Furthermore, Fenske reports that this is in line with an admirable goal within economics of 'taking the con out of econometrics' (2010, p. 180). Precisely for this reason, however, it is a shortcoming of the 'New African Economic History' that it has largely sidestepped the issue of data quality. A useful piece of general advice for cross-disciplinary work is that assumptions, data points and observations should roughly cohere with the state of knowledge in other disciplines. It could be argued that this is not only useful advice, but a fundamental principle.[27] Economists would be doing themselves a disfavour if the only criteria that are considered in evaluating the 'robustness' of historical arguments are those pertaining to econometric methods. The most fundamental prejudice against econometric studies is summarised as: 'garbage in, garbage out', indicating that the quality of the evidence needs to be justified cross-disciplinarily if the results are to be readily accepted.

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26 Platteau (2009, p. 676) argues that the fundamental problem in post-colonial Africa has been the 'fluid political setup dominated by unregulated factional competition as well as by the instability of ruling coalitions'.

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